

# Probability as typicality

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## Abstract

The concept of typicality refers to properties holding for the “vast majority” of cases and is a fundamental idea of the qualitative approach to dynamical problems. We argue that measure-theoretical typicality would be the adequate viewpoint of the role of probability in classical statistical mechanics, particularly in understanding the micro to macroscopic change of levels of description.

*Keywords:* Statistical mechanics; Typicality; Probability.

## 1. Introduction

The year 2006 marked the 100th anniversary of Ludwig Boltzmann’s death. He is justly celebrated as one of the greatest theoretical physicists of the XIXth century and a founding father of statistical mechanics (for a perspective, see Cercignani, 1998 and Uffink, 2004). Besides, his work influenced (directly or indirectly) the development of important fields of pure and applied mathematics: foundations of probability theory, the theory of stochastic processes (Brownian motion), ergodic theory and functional analysis (integral-differential equations).

He also had a significant impact on the philosophy of science. Firstly, he contributed to the unity of physics by attempting to reconcile the atomistic-mechanical view of microscopic dynamics with the macroscopic world of thermodynamics in the context of kinetic gas theory. Secondly, in that endeavor he had a clear view that progress in physics involves hypothesizing unobservable (but not inscrutable) “simple” material entities in order to explain the behavior of “complex” systems.<sup>1</sup> Thence his role in the “battle over the reality of the atom” (Wick, 1995), which was the cornerstone of that “unification” program and which served as a prelude to the

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<sup>1</sup>In stark contrast to the somewhat barren positivist stance of the “energeticists” (according to which energy was a kind of “primordial” entity) for which unobservables had no place in physics. Curiously, they regarded thermodynamics as their ideal, a theory which is full of unobservables (as in any deep physical theory, Bunge, 1967), entropy being one of the most intangible.

future unraveling of the structure of matter (see Brush, 1994). Thirdly, and closer to the focus of this article, in facing the sharp criticisms (the so-called “reversibility paradoxes”) against his program, he brought (with the contribution of Gibbs) probabilistic arguments to the center of stage in physics.

However, the use probabilistic concepts in kinetic theory <sup>2</sup> has generated a lot of controversy and misunderstanding, even after so many discussions and clarifications. For, how come that starting from Newton’s equations of motion for discrete particles one suddenly conjures up a continuous probability density in phase-space? <sup>3</sup> What exactly is the role and status of probability in classical statistical mechanics? Is it a concept totally alien to mechanics?

The difficulties Boltzmann had to face were not only conceptual but also technical due to the lack of adequate mathematical tools necessary to tackle (and even formulate) such hard questions in a clear, rigorous and meaningful way. For example, one must bear in mind that a probability *theory* proper was not yet available and there was (and still is!) some confusion regarding its status, as many people viewed it as a peculiar blend of physics and mathematics. <sup>4</sup> As von Plato’s (1991) pointed out, Boltzmann “was a XIXth-century theoretical physicist, not a XXth-century mathematician. Even so, he has been judged and interpreted according to mathematical concepts and standards that were not his. The mathematics of probability of the previous century being what it was, he was still able to achieve his end by reasoning that later developments confirmed” (p. 87).

That the problems of kinetic theory were deemed highly challenging (in fact, they are still open today) and important can be gauged by their inclusion as part of the 6th problem in David Hilbert’s famous list. It deals with the axiomatization of physical theories and, in particular, probability theory. <sup>5</sup> In his words (quoted in Corry, 1997, p. 121):

The investigations on the foundations of geometry suggest the problem:  
To treat in the same manner, by means of axioms, those physical sciences  
in which mathematics plays an important part; in the first rank are the  
theory of probabilities and mechanics.

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<sup>2</sup>Interestingly, the use of statistics in kinetic gas theory was inspired by the astronomer and demographer Adolphe Quêtelet, see Torretti, (1999), p. 180 and von Plato (1998), p. 73.

<sup>3</sup>It seems that the idea of studying a mechanical systems by means of a probability density on the set of initial data was pioneered by Poincaré in his “method of arbitrary functions”, see von Plato, 1983.

<sup>4</sup>According to Kac (1949), Poincaré used to joke about the confusing status of the central limit theorem, saying that “there must be something mysterious about the normal law since mathematicians think it is a law of nature whereas physicists are convinced it is a mathematical theorem” (p. 52) (the physicists were right here!). Maybe a trace of this confusion remains as many of the *theorems* of probability theory are labeled as “laws”, as in the “law of large numbers”, “the normal law”, “Kolmogorov 0-1 law”, etc. Incidentally, in 2006 a Fields Medal (the Nobel prize of mathematics) was awarded for the first time ever to a probabilist.

<sup>5</sup>Note that Hilbert considered probability (like geometry) as part of physics, not an uncommon view at the time.

As to axioms of the theory of probability, it seems to me desirable that their logical investigation should be accompanied by a rigorous and satisfactory development of the method of mean values in mathematical physics and in particular in the kinetic theory of gases.

In this paper we argue that *typicality* would be the adequate viewpoint of the role of probability in classical statistical mechanics.<sup>6</sup> Instead of indicating the presence of a random ingredient in the system, it is taken as a yardstick in probing the relative *size* of some sets of micro-states of interest (in particular, initial conditions) in the geometrical arena of phase-space. As discussed in section 2, this view is at the very heart of modern axiomatic probability theory, which is based on Borel and Lebesgue’s measure theory. Now, as the initial data are an integral part of any mechanical system, the use of probability to size up such data makes it less foreign to mechanics.

Generally speaking, a set is typical if it contains an “overwhelming majority” of points in some specified sense. In classical statistical mechanics there is a “natural” sense: namely sets of full phase-space volume. That is, one considers the Lebesgue-measure<sup>7</sup> on phase-space, which is invariant (by Liouville’s theorem) which, when “cut to the energy surface”, can be normalized to a probability measure; then sets of volume close to one are considered typical. We suggest that the focus on such (measure-theoretical) typical micro-states leading to the system’s macroscopic behavior (in an appropriate limit) underlines the role of probability in bridging the micro-macro levels of description, which is a basic aim of statistical mechanics. Besides, as discussed in section 3, probability as typicality has a long history of success, from celestial mechanics to the modern theory of dynamical systems.

We also think that, from this perspective, the use of probabilistic reasoning in classical statistical mechanics is more understandable. It could be conceived as part of the *zeitgeist* of the last decades of the XIXth when a revolutionary trend from quantitative to qualitative methods was taking place, pioneered by Poincaré, more or less at the same time as Boltzmann’s work in kinetic theory<sup>8</sup> (a field in which, by the way, Poincaré had a keen interest, his paper on the subject dating from 1894, see von Plato, 1991, p. 83).

Finally, in section 4 we discuss the use of probability as typicality in a remarkable achievement in non-equilibrium statistical mechanics, namely, Lanford’s theorem on the validity problem for Boltzmann’s equation.

## 2. Probability and Measure Theory

A. N. Kolmogorov solved part of Hilbert’s 6th problem in 1933 by his axiomatization of probability theory in his *Grundbegriffe* (Kolmogorov, 1957). The fundamental

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<sup>6</sup>The notion of typicality also plays a crucial role in the revamped version of Bohmian quantum mechanics, see for instance Dürr (2001).

<sup>7</sup>More generally, an absolutely continuous measure with respect to Lebesgue-measure.

<sup>8</sup>This would deserve a more thorough historical investigation. It is fascinating to wonder what these two quite different characters would had to say to each other as they most probably met at the 1904 St. Louis world fair (Cercignani, 1998, p. 145).

insight was that probability theory, going beyond its “elementary” part which deals with discrete sample spaces and reduces essentially to combinatorics, could be seen as a branch of the newly created *measure theory* of Lebesgue.

Although measure theory developed from internal problems in mathematical analysis, linked to the need to generalize Riemann’s integral in the context of Fourier’s series (Kahane, 2001, Hoare and Lord, 2002), it also had ancient geometric roots. As made clear in Lebesgue’s 1902 doctoral dissertation, titled *Intégrale, longueur, aire*, measure theory is conceived as a (very abstract) generalization of basic geometrical notions of size: length, area and volume (Choquet, 2004).

Recall that in modern mathematical language a *measure space* is a triple  $(\Omega, \mathcal{F}, \mu)$  where:  $\Omega$  is an arbitrary set (usually equipped with some natural topology);  $\mathcal{F}$  is a collection of subsets of  $\Omega$ , called *measurable subsets*, carrying the structure of a  $\sigma$ -algebra (i.e., it is closed under denumerable set-theoretical operations); and  $\mu$  is a non-negative countably additive set function on  $\mathcal{F}$ .<sup>9</sup> Kolmogorov noticed that probability theory could be perfectly couched in this framework. A *probability space* is defined as a triple  $(\Omega, \mathcal{F}, \mathbf{P})$ , where  $\Omega$  is the “sample space”,  $\mathcal{F}$  the collection of “events” and  $\mathbf{P}$  is a finite measure normalized to one,  $\mathbf{P}(\Omega) = 1$ . If  $A$  is an “event” (a measurable set)<sup>10</sup>, then  $\mathbf{P}(A)$  is its probability “of occurrence”. Also, “random variables” are identified to measurable functions and expectations to Lebesgue-integrals with respect to the given probability measure.<sup>11</sup>

Kolmogorov’s axiomatization (which today reached almost universal acceptance) has an enormous significance: not only it gave the seal of maturity and mathematical respectability to the discipline, which has been expanding relentlessly since then, as it greatly clarified its nature. Most importantly, it became clear, once and for all, that probability theory is a branch of pure mathematics, like group theory, geometry, linear algebra, etc. Hence it has many models in the set-theoretic sense so that the expression “taken at random” has different meanings depending on the specified probability space. This helped dissolving many paradoxes that plagued probability theory, like Bertrand’s, which were linked to a careless use of that phrase. Freed from any previous commitment to an “interpretation” (be it frequentist, subjectivist, propensity, etc) the theory could be developed autonomously. Moreover, once its nature is so elucidated one can examine and criticize any proposed interpretation or application of probability theory to the real-world.

For the better or worse, probability theory has kept the old jargon of its pre-axiomatized era. Though it is debatable whether such notions as “sample”, “event”,

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<sup>9</sup>A pair  $(\Omega, \mathcal{F})$  is called a *measurable space*, meaning that it can carry different measures.

<sup>10</sup>Notice that, as  $\mathcal{F}$  is usually strictly smaller than the set of all subsets of  $\Omega$ , some subsets of the sample space may have no probability at all.

<sup>11</sup>Of course, Kolmogorov’s legacy in probability theory is much wider: he clarified the concept of independence (there is a joke saying that probability theory is “just” measure theory plus the concept of independence in the same way that complex analysis as “just” analysis in two-variables plus  $\sqrt{-1}$ ) and conditioning; established a host of now-classical limit theorems for sums of independent random variables; and made seminal contributions to the theory of continuous-time stochastic processes (see Mazliak, Chaumont and Yor, 2004).

“occurrence”, “trials”, “favorable event”, “experiment”, etc may or not have some heuristic or pedagogic value, the fact is that they are not, strictly speaking, part of probability theory. In particular, there is nothing intrinsically random about “random variables”: they are just real-valued measurable functions, those one expects to find in real analysis (for instance, the continuous functions).

The old phraseology has to be used with great care, particularly in applications. For instance, coin-tossing is usually taken as the epitome of a random phenomenon. However, *real* coin-tossing is a purely mechanical process that should in principle be modeled using rigid-body Newtonian dynamics plus the initial conditions; and its relation to randomness and unpredictability is a non-trivial matter (see Diaconis, Holmes and Montgomery, 2005).

### 3. Negligible sets

As a measure on a set can be viewed as giving the size of some of its subsets, so a probability measure can be conceived as giving their relative sizes. Moreover, a measure allows us to take some sets as being “small”, “exceptional”, “atypical” or “negligible” and hence ignored in some specific contexts.

For the record, we note that there are at least two other common notions of size (and hence, of typicality) in mathematics: *cardinality*, giving the number of points (or power) of a set and a topological notion, called *genericity*, particularly useful in dynamical systems theory. We won’t discuss them here as our focus is on the *measure-theoretical* notion of typicality.<sup>12</sup> Roughly speaking, a property  $\mathcal{P}$  on a measure space is *typical* if the set  $\wp^c = \Omega - \wp$  of its *exceptions* has “small” measure (in particular,  $\wp^c$  has to be a measurable set) that is,  $\mu(\wp^c) \leq \epsilon$  for some “tolerance”  $0 \leq \epsilon \ll 1$ , where  $\wp = \{\omega \in \Omega : \mathcal{P}(\omega)\}$ . It is important to bear in mind that, as Goldstein (2001) observed, typicality “plays solely the role of informing us when a set  $E$  of exceptions is sufficiently small that we may in effect ignore it” (p. 53), so one could conceive of some weaker notion of typicality, without the additional measure-theoretical structure.<sup>13</sup>

An important and natural example are sets of measure zero (also call null sets), corresponding to  $\epsilon = 0$ .<sup>14</sup> In measure theory it is known that changing a measurable function on a null set (which can be uncountable) won’t affect the Lebesgue integral of that function. This suggests the notion of a property holding *almost-everywhere*, meaning that it holds outside a set of measure zero. In the analogous situation in probability theory one says it holds *almost-surely* or *with probability one*.

Thus, in measure/probability theory a property is said to hold even if it has infinitely (even uncountably) many exceptions, as long as these form a set of small measure/probability. For instance, in 1909 Borel obtained the first proof of a strong

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<sup>12</sup>The relationships between these three notions are quite complex, see Oxtoby, 1971.

<sup>13</sup>A possibility that comes to mind is outer measure, which is only sub-additive and defined on all subsets of  $\Omega$ .

<sup>14</sup>This is the strongest measure-theoretical notion of typicality and historically the concept of null set predates measure theory proper, see von Plato (1983), p.44.

law of large numbers by showing that Lebesgue almost every real number in  $[0, 1]$  is normal to base 10, meaning that any block of  $k$  digits appears with asymptotic frequency  $10^{-k}$  in its decimal expansion (and the same holds for any base). So, though there are uncountably many non-normal numbers, as seen through the lenses of Lebesgue measure (so to speak), “all” real numbers are normal. However, as remarked by Kac (1949), “as is often the case, it is much easier to prove that an overwhelming majority of objects possess a certain property than to *exhibit* even one such object” (p. 18) and to this day no one knows whether such fundamental constants as  $\pi$ ,  $e$ ,  $\sqrt{2}$  are normal (to any base!).<sup>15</sup> We stress though, that the whole point of typicality arguments is that, for certain purposes, one would not need such detailed information.

The relation of typicality to probability is subtle, as the following example illustrates. Consider the set of all binary strings of say, 200 bits. For example, one could think of it as the sample space associated to the tossing 200 ideal fair coins (heads=1, tail=0). Now, in the overwhelming majority of strings the total number of heads is between 50 and 150 (i.e., within 50 of the mean 100) and we may declare such strings as typical. Of course, the string 11...11, consisting of 200 heads, is not typical. However, it has the very same (and very small) probability, namely  $2^{-200}$ , as each and every one of the  $2^{200}$  possible strings.<sup>16</sup>

It seems that the first typicality-like arguments appeared in the field of celestial mechanics, in connection with the classical problem of the stability of the solar system. This came about after the rather slow realization that one could not expect to “explicitly” solve most differential equations and, moreover, that such a solution might be uninformative. A clear illustration of this state of affairs is the Newtonian gravitational three-body problem. Though this is not an integrable system (in the precise sense of Hamiltonian mechanics) it has an “explicit” or “analytic” solution, which was found by Sundman in 1909 (see Henkel, 2001). He obtained a convergent series solution, valid for all times,<sup>17</sup> but whose rate of convergence is so slow<sup>18</sup> as to render it virtually useless to extract interesting information about the long-time behavior of the system.

In that long historical trend in mathematical-physics, which eventually led to the switch from quantitative to qualitative methods pioneered by Poincaré, Lyapunov and others (see Laskar, 1992 and Chenciner, 1999), the focus changes from a detailed analysis of *individual* solutions of a given system to the study of whole *families* of them (and of families of systems). As the 1994 Fields medalist J.-C. Yoccoz

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<sup>15</sup>By the way, normality was initially taken to be a reasonable definition of randomness for sequences of digits. However, it was abandoned as it was proven by Champernowne in 1933 that the number 0.12345678910111213... (the concatenation of the positive integers) is normal to base 10. For a discussion of randomness for sequences, see Volchan, 2002.

<sup>16</sup>This is a version of the so-called “paradox of randomness”, see Volchan, 2002.

<sup>17</sup>And which fulfills precisely the requirements stated in the celebrated king Oscar II prize problem, see Barrow-Green, 1997.

<sup>18</sup>It is estimated that  $10^{8,000,000}$  terms of the series would be necessary to reach the standard of accuracy in modern ephemeris calculations, see Goroff, 1992, p. I25

puts it: “Broadly speaking, the goal of the theory of dynamical systems is, as it should be, to understand most of the dynamics of most systems.” (Yoccoz, 1995, p.247). Note that as Sundman’s example shows, resorting to qualitative methods (including statistics) is not necessarily linked to the huge number of equations one is dealing with nor to ignorance (or imprecision) of initial conditions, notwithstanding a common claim in statistical mechanics textbooks.

One of the earliest examples of the qualitative approach to dynamical problems is the famous *Poincaré’s recurrence theorem* which Poincaré himself called “stabilité a la Poisson” (and is also known as “Poincaré-reversibility”<sup>19</sup>). This remarkably simple result, which appeared in his memoir on the three-body problem (1890), is a forerunner of the ergodic theorems and is one of the few global results in dynamical systems theory.

Its measure-theoretical version (not the original one, as measure theory was not yet available) says the following. Consider a flow  $\mathbf{T}_t$  (e.g., associated to the solutions of a differential equation) on a set  $\Omega$  and  $\mu$  a finite *invariant* measure. Then, almost all points of  $\Omega$  are recurrent, that is, the orbit of every initial condition  $x$  outside a set of  $\mu$ -measure zero will eventually come arbitrarily close to  $x$  (such orbits were called “stable”).

Now, by normalizing  $\mu$  to a probability measure the theorem can be rephrased thus: the flow is recurrent with probability one or that an initial data “taken at random” is recurrent. According to von Plato (1991), in this result “for the first time, a property is ascribed to mechanical systems with probability one. Exceptions to the problem are not impossible but have probability zero” (p. 83).<sup>20</sup> However, as there is no intrinsic randomness in the dynamics nor in the initial data, what the theorem really seems to convey is that the property of recurrence is typical with respect to the measure  $\mu$ . In other words, it holds for “the vast majority” of initial states. Poincaré himself seems to corroborate this view as, according to Barrow-Green (1997), he claimed that “stable trajectories would *outnumber* the unstable, in direct analogy with the irrational and rational numbers” (p. 87, our emphasis).

The power of Poincaré’s theorem stems from its being a very general global result that does not require detailed knowledge of the motion. Of course, it does presuppose the long-time *existence* of solutions! This can be a matter of concern due

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<sup>19</sup>Which is on of several concepts of reversibility. A nice discussion of the subject can be found in Illner and Neunzert, 1987.

<sup>20</sup>Incidentally, from discussions of earlier work of Gylden (1888) on the related problem of planetary mean motion came out the first clearly articulated methodological principle linked to the negligibility of null sets, due to Felix Bernstein (1912). He called it “the axiom of the limited arithmetizability of observations” according to which (quoted in von Plato, 1998, p.63):

When one relates the values of an experimentally measured quantity to the scale of all the reals, one can exclude from the latter in advance any set of measure zero. One should expect only such consequences of the observed events which are maintained when the observed value is represented by another one within the interval of observation.

to the possible existence of initial data leading to “singularities”, that is, obstructions to the extension of solutions. For instance, in the Newtonian gravitational  $N$ -body system the planets (idealized as point particles) can collapse or even disperse to infinity in finite time. Here again, a typicality argument comes to the rescue as one hopes to prove that such “catastrophes” are rare,<sup>21</sup> in the sense that the set of initial conditions leading to them is negligible (a very hard problem which is open in the general case, see Saari, 2005).<sup>22</sup>

The analogous problem in kinetic theory is fortunately much simpler. Consider the usual “billiard-balls” model of a classical gas as made of hard impenetrable spheres (free flow plus elastic shocks). There is an ambiguity here as how to extend the flow past an instant where three or more particles collide. However, the initial conditions leading to such situation form a measurable subset of lower dimension in phase-space, being therefore of Lebesgue measure zero (a corresponding result holds for initial data leading to infinitely many collisions in finite time). Therefore the dynamics is well defined (for all time) for Lebesgue almost every initial condition (Cercignani, Gerasimenko and Petrina, 1997).

As is well known, the recurrence theorem was used by Poincaré and Zermelo as a formidable objection to Boltzmann’s attempt to reconcile the irreversible character of macroscopic phenomena with the reversible nature of the microscopic Newtonian dynamics of a gas.<sup>23</sup> In fact, it applies to a classical gas of  $N$  particles in an isolated bounded container. But a gas initially restricted to half of the container will, if left to itself, diffuse until it occupies the whole volume while one never observes it return spontaneously to that initial situation. This is a version of the “recurrence paradox” (or *Wiederkehrwand*) which, together with the “reversibility objection” (or *Umkehrwand*), still is a source of contention among physicists.<sup>24</sup>

#### 4. Typicality in Statistical Mechanics

The crucial point in coming to terms with the reversibility conundrum is the realization that one is examining a system at two very different *levels*. In this sense, Poincaré’s theorem is a clue indicating that any hope to derive in a mathematically rigorous way the macroscopic irreversible equations (like Boltzmann’s, Euler’s or Navier-Stokes) from a microscopic reversible dynamics will involve some kind of idealized limit in which the number of particles goes to infinity and under which

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<sup>21</sup>This also exemplifies the fact that what is taken as negligible in some contexts may not be so in others: such “singularities” as collisions of celestial bodies are obviously of considerable astronomical interest. Another example are phase-transition points in equilibrium statistical mechanics.

<sup>22</sup>We should mention yet another famous (and much harder) qualitative result in Hamiltonian mechanics: the *KAM theorem* also can be seen as a typicality result. Roughly, it says that for a sufficiently small perturbation of an integrable Hamiltonian system (plus technical hypothesis) the set of initial conditions leading to quasi-periodic orbits is a set whose complement has small Lebesgue measure, which tends to zero as the perturbation tends to zero (Pöschel, 2000).

<sup>23</sup>Particularly against his “H-theorem”, see Illner, 1988.

<sup>24</sup>The author had the opportunity to witness the extant disagreements on the occasion of a round table on irreversibility at the STATPHYS-20 conference in Paris (1998), having professors Ruelle, Lebowitz, Prigogine and Klein as panelists.



there is a “loss of Poincaré-reversibility”.

That some limit procedure is necessarily involved here was clearly stated in Hilbert’s formulation of the 6th problem which, while recognizing Boltzmann’s intuition, reads:

Thus Boltzmann’s work on the principles of mechanics suggests the problem of developing mathematically the limiting processes, there merely indicated, which lead from the atomistic view to the laws of motion of continua (cited in Wightman, 1976, p 148).

A related observation is that under such change of levels of description there is a dramatic “decimation” of degrees of freedom: while the micro-state has  $6N$  (of the order of  $10^{23}$  for a gas in normal conditions) degrees of freedom (all the particle’s positions and velocities) the macro-state usually involves few variables (say, density, pressure and temperature).<sup>25</sup>

This reduction suggests that some kind of averaging procedure should be involved and which, together with the large  $N$  limit, points to the role of statistics. However, as there is no intrinsic randomness in the system at hand the use of statistics and probability might be better understood through typicality. In particular, the taking of averages does not necessarily implies any randomness: it could just mean that details are not important (Bunge, 1988) which is, after all, the basic philosophy behind the qualitative approach. Once more, it highlights

...what the statistical aspect of statistical mechanics really is, namely, the assertion of *circumstances which may be neglected* (Truesdell, 1966, p.77).

It then seems that the general procedure needed to realize Boltzmann’s program in the lines envisioned by Hilbert goes as follows (Guerra, 1993, Lanford, 1976). Consider a “physically significant” macroscopic state-function  $F$  (usually linked to locally conserved quantities). A fundamental insight of Boltzmann is that there are many different micro-states  $\omega \in \Omega_{\Lambda,N}$  compatible with the same macrostate  $F$  in the following sense. One partitions the one-particle phase-space into macroscopically small but microscopically large cells  $\Delta_\alpha$  and specifies the number  $n_\alpha$  of particles lying in each cell when the system is in macro-state  $F$  (and maybe with additional specifications, like energy, etc, according to desired macroscopic description of the system, be it kinetic, hydrodynamic, etc ). The micro-states corresponding to those specifications will have similar density/velocity profiles and define the set  $\Gamma_F \subset \Omega_{\Lambda,N}$ .

Let now  $F(t)$  be the macro-state at time  $t$  evolved from  $F$  according to the macroscopic phenomenological equations (kinetic, hydrodynamic, etc) while  $\omega(t)$  is the micro-state at time  $t$  evolved according to Hamilton’s equations from an  $\omega \in \Gamma_F$ . Then, one would like to prove that for the *overwhelming majority* of initial micro-states  $\omega \in \Gamma_F$  and *in an appropriate limit* (kinetic, hydrodynamical, etc) one obtains

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<sup>25</sup>However, in non-equilibrium cases one deals with corresponding fields, which are strictly speaking, infinite dimensional vectors.

$\omega(t) \in \Gamma_{\mathbf{F}(t)}$ . It turns out that this is very hard to do in “realistic”<sup>26</sup> scenarios. In the following we restrict our discussion to a case in which a spectacular breakthrough was achieved namely, Lanford’s theorem on the *validity problem* for Boltzmann’s equation.

Recall that Boltzmann (building on previous work of Maxwell) wrote down his equation in 1872 to describe the time evolution of the single-particle distribution function  $f_t(\mathbf{r}, \mathbf{v}) = f(\mathbf{r}, \mathbf{v}, t)$  for a dilute gas of  $N$  identical hard impenetrable spheres of mass  $m$  and diameter  $a$  in a region  $\Lambda \subset \mathbb{R}^3$ . It is a non-linear integral-differential equation which reads (with no external force field)

$$\frac{\partial}{\partial t} f_t(\mathbf{r}, \mathbf{v}) + \mathbf{v} \cdot \nabla_{\mathbf{r}} f_t(\mathbf{r}, \mathbf{v}) = Q(f_t, f_t) =$$

$$Na^2 \int_{\mathbb{R}^3} d^3 \mathbf{v}_1 \int_{\hat{\mathbf{n}} \cdot (\mathbf{v} - \mathbf{v}_1) \geq 0} d\hat{\mathbf{n}} \hat{\mathbf{n}} \cdot (\mathbf{v} - \mathbf{v}_1) [f_t(\mathbf{r}, \mathbf{v}_1') f_t(\mathbf{r}, \mathbf{v}') - f_t(\mathbf{r}, \mathbf{v}_1) f_t(\mathbf{r}, \mathbf{v})] \quad (1)$$

where  $(\mathbf{v}, \mathbf{v}_1)$  and  $(\mathbf{v}', \mathbf{v}_1')$  are, respectively, the incoming and outgoing velocities; and the so-called collision operator  $Q(\cdot, \cdot)$ , summarizes the effects of the shocks.

This is one of the most successful equations of physics, with a broad range of applications and a challenging subject for mathematicians. In particular, and despite some remarkable recent advances, the global existence and uniqueness of well-behaved solutions is still an open problem. However, Boltzmann “derived” his equation by a straightforward heuristic analysis of the collision process using some bold simplifying assumptions. Notably, he only considered binary uncorrelated collisions (no two particles collide more than once),<sup>27</sup> which boils down to the famous “molecular chaos hypothesis” or *Stosszahlansatz*.

But, how can a *discrete*  $N$ -particle classical gas be described by a *continuous* one-particle distribution function? Sometimes one reads that  $N \int f(\mathbf{r}, \mathbf{v}, t) d^3 \mathbf{v} d^3 \mathbf{r}$  is the number of particles of the gas in the infinitesimal region  $d^3 \mathbf{v} d^3 \mathbf{r}$  around the one-particle phase-space point  $(\mathbf{v}, \mathbf{r})$  at time  $t$ . But that cannot be: when the gas is in the micro-state  $\omega(t) = \mathbf{T}_t(\omega(0)) = (\mathbf{q}_1(t), \mathbf{v}_1(t), \dots, \mathbf{q}_N(t), \mathbf{v}_N(t))$ , the number of particles in, say, a rectangular parallelepiped  $\Delta \subset \Lambda \times \mathbb{R}^3$ , is given by

$$\sum_{i=1}^N \mathbb{I}_{\Delta}(\mathbf{q}_i(t), \mathbf{v}_i(t)), \text{ which is an integer.}$$

So, instead of introducing a random ingredient in the system, usually justified on the basis of “ignorance” or “imprecision” on the initial data, one could take the viewpoint that  $f_t(\cdot, \cdot)$  gives a macroscopic description of the gas as a continuum medium, from which one gets, for instance, the hydrodynamic fields of mass, momentum and kinetic energy densities respectively:  $\rho(\mathbf{r}, t) = mN \int_{\mathbb{R}^3} f_t d^3 \mathbf{v}$ ,

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<sup>26</sup>That is, taking classical mechanics as the microscopic model. The analogous problem is much more complete for stochastic lattice systems, see Spohn, 1991 and Boldrighini, 1996.

<sup>27</sup>Ternary and higher order collisions are of two kinds: “genuine” (i.e., simultaneous) which, as we have seen, are negligible; and “correlated successive binary collisions” carrying memory effects, which are crucial in the study of *dense* fluids, see Cohen, 1993.

$$\rho \mathbf{u}(\mathbf{r}, t) = mN \int_{\mathbb{R}^3} \mathbf{v} f_t d^3 \mathbf{v} \text{ and } e(\mathbf{r}, t) = mN \int_{\mathbb{R}^3} \frac{1}{2} \mathbf{v}^2 f_t d^3 \mathbf{v}. \quad {}^{28}$$

In other words,  $f$  gives an approximate (“coarse-grained” or “reduced”) description of the system which in Lanford’s analysis is made precise as follows (Lanford, 1983): a micro-state  $\omega = (\mathbf{q}_1, \mathbf{v}_1, \dots, \mathbf{q}_N, \mathbf{v}_N)$  is said to be “close” to  $f(\mathbf{r}, \mathbf{v})$  when

$$F_\Delta(\omega) = \frac{1}{N} \sum_{i=1}^N \mathbb{I}_\Delta(\mathbf{q}_i, \mathbf{v}_i) \approx \int_\Delta f(\mathbf{r}, \mathbf{v}) d^3 \mathbf{r} d^3 \mathbf{v}, \quad (2)$$

which approximation becomes exact only in an appropriate limit. It was H. Grad who suggested that the limit involved here should reflect (in idealized form) the physical situation of a dilute gas where the diameter of the particles is much smaller than the mean free path, so that particles rarely meet. This translates to  $N \rightarrow \infty$  and  $a \rightarrow 0$  with  $Na^2$  converging to a fixed non-zero constant, called the *kinetic limit* or, as suggested by Lanford (1976), *Boltzmann-Grad limit* (p. 79). Note the total volume occupied by the particles is of order  $Na^3$  which goes to zero, such that one is dealing with an “infinitely diluted gas”.

Now, as one cannot expect the approximation condition to hold for *all* micro-states, one resorts to a typicality argument to at least guarantee that it will hold for the “vast majority” of them. Introduce then the following notion: a sequence  $\{\mathbf{P}_N\}_{N \geq 1}$  of probability measures on phase-space <sup>29</sup> is an *approximating sequence* for  $f$  if, for all  $\epsilon > 0$ ,

$$\mathbf{P}_N[\omega \in \Omega_{\Lambda, N} : |F_\Delta(\omega) - \int_\Delta f(\mathbf{r}, \mathbf{v}) d^3 \mathbf{r} d^3 \mathbf{v}| > \epsilon] \rightarrow 0, \quad (3)$$

in the Boltzmann-Grad limit. This renders precise the micro to macroscopic change of description. <sup>30</sup>

We can now state Lanford’s theorem, in a very simplified form, as follows:

Let  $f_t(\mathbf{r}, \mathbf{v})$  be a mild solution of Boltzmann’s equation with initial data  $f_0(\mathbf{r}, \mathbf{v})$ . Under some technical hypotheses, if  $\{\mathbf{P}_N\}_{N \geq 1}$  is an approximating sequence for  $f_0$ , then there exists a  $t_0 > 0$  such that the time-evolved (under the hard sphere dynamics) sequence  $\{\mathbf{P}_N \circ \mathbf{T}_{-t}\}_{N \geq 1}$  is approximating for  $f_t(\mathbf{r}, \mathbf{v})$ , for all  $t \in [0, t_0]$ .

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<sup>28</sup>Incidentally, the derivation of hydrodynamic (Euler, Navier-Stokes) equations from Boltzmann’s equation has a long tradition going back to Hilbert and is nowadays quite complete (see Esposito, Lebowitz and Marra, 1999). The much harder problem of deriving them from Newton’s equations, pioneered by Morrey’s work in the fifties, is still essentially open. Recently, Olla, Yau and Varadhan (1993) were able to get it but at the cost of introducing some unwarranted assumptions (of technical nature), such as adding a small stochastic noise to Newton’s equations.

<sup>29</sup>Each  $\mathbf{P}_N$  is defined on the corresponding phase-space  $\Omega_{\Lambda, N}$  which should also be modified to exclude overlapping of the particles.

<sup>30</sup>The concept of approximating sequence also has built-in the molecular chaos property, see Cercignani, Illner and Pulvirenti, 1994, p. 92.

It is important to realize that Lanford's theorem does not say that Boltzmann's equation holds "on the average" but that it describes the macroscopic behavior of the gas (in the Boltzmann-Grad limit) for the "vast majority" of micro-states which are initially close to  $f_0$ , at least for some small time interval. The result can be seen as a *law of large numbers* as the conclusion is that for all  $\epsilon > 0$  and in the Boltzmann-Grad limit, for  $t \in [0, t_0]$ :

$$\mathbf{P}_N[\omega \in \Omega_{\Lambda, N} : |F_{\Delta}(\mathbf{T}_t(\omega)) - \int_{\Delta} f_t(\mathbf{r}, \mathbf{v}) d^3\mathbf{r} d^3\mathbf{v}| > \epsilon] \rightarrow 0. \quad (4)$$

The rather technical proof, based on a careful analysis of Liouville's equation and the BBGKY-hierarchy (see Cercignani, Illner and Pulvirenti, 1994 or Spohn, 1991), requires a "proper balance between dynamics and probability" (Grad, 1949). As it turns out, Lanford's proof is a local result, that is, valid only for a very short (but strictly positive) time-interval, of the order of one-fifth of a mean free path. Though this is a severe shortcoming for applications, it does not diminish the great conceptual impact of the result. As remarked by Illner (1988) "the limiting evolution given by the Boltzmann equation is irreversible even on such a small time interval" (p.158). Of course a major open problem is to improve the time scale of the theorem.<sup>31</sup> As for the more realistic case of dense fluids the situation is much harder (for a discussion, see Cohen, 1997).

Lanford's theorem is thus the first and "remains the only rigorous result on the scaling limits of many-body Hamiltonian systems with no unproven assumptions" (Yau, 1998, p.194). It can be seen, even with all its restrictions, as the realization, after nearly a hundred years, of Boltzmann's intuitions as made precise by Hilbert, Grad, Morrey and many others. According to Gallavotti (1999), "this is an important confirmation, mathematically rigorous, of Boltzmann's point of view according to which reversibility, and the corresponding recurrence times, is not in contradiction with the experimental observation of irreversibility" (p. 35).

#### 4. Conclusions

The standard textbook justification for the use of probability in classical statistical mechanics follows an operationalistic view. First, as the microscopic dynamics itself is non-random, any randomness is shifted to the initial conditions. Then, goes the argument, due to our inability to either solve the huge number of equations or measure with precision the initial data, we have to resort to statistics. In other words, human limitations are the basis for the justification.

We find that untenable and argued in favor of an alternative viewpoint based on the notion of typicality. This is not to say that other viewpoints would not be more adequate in other contexts, but that in the case of *classical* statistical mechanics typicality seems to be more natural. It does not invoke any randomness (ontological or epistemological), which is consistent with the kind of classical system

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<sup>31</sup>Results in this direction were obtained for a rarefied gas in all space, under additional hypothesis on the initial data of Boltzmann's equation (see Illner and Neunzert, 1989).

at hand. Also, as we have seen, measure-theoretical typicality arguments have been used successfully in the qualitative study of Hamiltonian systems in many other contexts. The idea is to obtain results valid for the “overwhelming majority” of initial conditions. In this sense, as the initial conditions are an integral part of mechanical systems, probability-as-typicality is not that foreign to mechanics.

Of course, one *does* need an ingredient “outside of mechanics” when trying to bridge the micro and macro levels of description which is the main goal of statistical mechanics. Boltzmann had the intuition that some idealized limit would be involved and, as Lanford’s analysis illustrates, in classical statistical mechanics probability measures enter as crucial level-connecting concepts in realizing that goal in a rigorous way. Also, Lanford’s theorem does imply that for some “rare” initial conditions the corresponding macroscopic dynamics will not follow the observed behavior. However, because they are rare in the measure-theoretic sense used in statistical mechanics, the idea is that such data can be ignored. This is in the spirit of measure/probability theory in which a property is taken to hold true when the set of exceptions is rare in the sense of having very small measure/probability (even if such set is large in terms of cardinality). In this sense, we suggest that probability-as-typicality is a way to express, in a mathematically precise (albeit idealized) way, the validity of the macroscopic laws and their compatibility with the atomistic-mechanical microscopic model.

As usual in mathematical-physics, the idealizations are the unavoidable price to pay in exchange for rigorous analysis. Another example: in equilibrium statistical mechanics in order to define phase-transitions points as singularities of the partition functions one has to take the thermodynamic limit. This does not mean that real (finite) physical systems do not exhibit phase-transitions but that the idealization expressed by the limit  $N \rightarrow \infty$  helps in better handling the problem mathematically than with a finite system.<sup>32</sup>

Many tough questions are still to be addressed. For example, as typicality is relative to the measure used, how one justifies a particular choice? Under what criteria? Moreover, what about other notions of typicality? Also, as we mentioned, typicality is not enough to decide when a given initial data belong to a desired subset, which is very important regarding the trend to equilibrium issue. And of course, typicality does not avoid the need of rigorous analysis and proof.

Finally, though the notion of probability-as-typicality is not new, it is seldom articulated clearly and it should be allowed more space in the debates on the foundations of statistical mechanics. We also think that the appearance of that notion at approximately the same time in the theory of dynamical systems and statistical mechanics deserves a deeper investigation as it could be seen as a symptom of that broad historical transition from quantitative to the qualitative methods.

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<sup>32</sup>As remarked by the late mathematical-physicist R. Dobrushin (1997), “infinity is a better approximation to the number  $10^{23}$  than the number 100” (p.227).

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